SCIENCE

A WEEKLY NEWSPAPER OF ALL THE ARTS AND SCIENCES.

PUBLISHED BY N. D. C. HODGES, 874 BROADWAY, NEW YORK.

TENTH YEAR.
VOL. XX. No. 508.

OCTOBER 28, 1892.

SINGLE COPIES, TEN CENTS.
$3.50 PER YEAR, IN ADVANCE.

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Entered at the Post-Office of New York, N.Y., as Second-Class Mail Matter.

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SCIENCE

NEW YORK, OCTOBER 28, 1892.

THE HUMMING-BIRD'S FOOD.

BY MORRIS GIBBS, M.D.

This article refers to the ruby-throat, the only representative of this interesting family in our State. Much has been written regarding the food of this species, and yet I am satisfied that but few accurate notes have been offered to the readers. The writer offers observations taken with a view to learning of the feeding habits, and does not pretend to assert that others' notes, however conflicting, are not correct. Locality has everything to do with the habits of birds, and the requirements of the same species may differ vastly in a slight variation, either in latitude or longitude. Again, the resources of a region may radically alter the food habits of any and all animals. Certain it is, that my observations convince me, contrary to all writings that I have seen, that the food of the ruby-throated humming-bird is mainly honey, and that these little fellows do not rely to any extent on an insect diet.

Years ago I captured several in our flower-garden with my insect net, and, in accordance with the views of all books read, they were offered insects as food, but invariably completely ignored everything of this nature set before them. No matter whether I gave them the liberty of a large room or confined the frightened creatures in my hand or a small box, the result was invariably the same; all insect food was refused, whether small beetles, or even those minute flies or gnats, often common about honey-producing flowers. However, on releasing the captive, it would immediately visit the flowers, and appear to revel in the exploration of the deep recesses of the fuchsias and trumpet-creepers. One immature specimen that I caught would sip sugar-water from my hand, and even protrude its delicate tongue for the sweets to be so easily had. This young one was so very unsophisticated that it had to be taught regarding the honey-water, by dipping its tiny, slender beak into the sticky mass, after which it quickly learned. The old ones only fluttered in my hand, and would not eat, but would apparently enjoy that which was forced into their bills. But, left to themselves and watched secretly, they could be seen indulging in the sweets provided for them. If held carefully and an insect forced between their mandibles, they invariably ejected it with a snap of the bill and a side jerk of the head.

Of the wild flowers of Michigan, there are many species which the hummers visit regularly, but as nearly all of these flowers are so far from my residence, it follows that my observations are mainly made from our house-plants and garden flowers. Of all of the uncultivated species that I know, the flowers of the wild crab-apple are most sought after by the ruby-throat, and during the season, about the middle of May, a hundred birds may be seen in a few hours about a group of these trees. There are very few insects on the crabs, and in wet days none, and yet the hummers swarm about. They must come alone for sweets. One point in relation to my theory of the hummer's love for honey would seem to receive a challenge, and it is, that the ruby-throat rarely hovers over the common red and white clover. Now, as we know, red clover is one of the sweetest of flowers, and a head is agreeable to anyone's palate, while the white clover is a great favorite with the honey-bee. My reply to this is, that the individual flower is too small for the ruby-throat's attention.

On our piazza in the city are a number of house-plants, some growing in a hanging-box, others in pots on a stand, while several species of out-door perennials and annuals flourish in a bed just below, and a large creeper clambers near. It is safe to say that from early morning till evening twilight there will be an average of one visit every half-hour by the hummers to this collection. So unsuspicious have they become that one can study them at a yard's distance. One advantage in observing them is that they always make their presence known by their pleasant humming and a faint, sharp chirp; thus warning one when to lay aside the book and watch their movements.

On first appearing, they immediately dash towards the fuchsias, which are their greatest attraction, and the next best is the trumpet-creeper, and then the selection appears to them indifferent, as the pelargoniums, nasturtiums, morning-glories, and others are visited indiscriminately. However, the fuchsias are first choice, and, wondering at their preference, I examined the blossoms thoroughly for insects and sweets. In very few cases, and at rare intervals, I found small insects, as no others can reach the heart of the flower; but in every case I met with a most refreshing nectar,—to be sure, in very small quantity to us, but to a hummer, a most plentiful supply. Let my readers pluck a full-blown fuchsia blossom, and cutting into the calyx near the stem end, apply the part to the tip of the tongue, and they will be fully convinced why the hummer is partial to this beautiful pendant flower.

Thinking to test their fondness for sugar, some was dissolved and then dropped deeply into the blossoms of the creeper. In the course of the hour, in their rounds, the busy birds found the bait, and fully thrice the amount of time was spent on the extra-sweetened flowers as was occupied over those of nature's honeying. The sweetening attracted many insects in the course of the day, principally ants and small flies and gnats, but not one instance of their capture could I detect, although careful record of the number of insects in each flower was kept, and the flower examined after each bird departed.

The movements of the hummers when visiting a bed of flowers are interesting. With a dash it is among us with the characteristic impetuosity of its kind, but it is not then detected by the ear, as the noise of a flying bird is but slight and not always heard. It is when the bright, red-throated fellow stops in mid-air that we hear his rapidly-vibrating wings, always loudest when he makes a sudden side-movement from flower to flower. Selecting a flower, after a second's inspection of his surroundings, a rush is made toward it at a very rapid rate, but just as we think he will fly past or against the blossom, he stops—stops instantly. In the fraction of a second he introduces his tiny, but long, slim bill into the heart of the flower, and then is away to the next. The swiftness with which this delicate bird travels about, exploring hundreds of flowers each hour of the day, and from early morning till twilight is truly a marvel.

At each insertion of the tiny beak, his mobile tongue is thrust out and from side to side, and the sweets, and, I think, some pollen, are drawn into its mouth. The tip of the tongue is peculiarly and beautifully constructed for this purpose, and with the perfect adaptability of its slender, delicate bill, the bird is endowed with the means of securing sweets, possessed by no other group of birds.

In conclusion, I will say that I have carefully dissected many humming-birds, both old and young, but have never found anything to convince me that the birds lived on insects. It may be that at times when flowers are scarce some species of insects are captured, but I am satisfied that in season, when flowers are abundant, that the ruby-throat of Michigan lives on honey.

Kalamazoo, Michigan.

THE ORIGIN OF THE CAVE FAUNA OF KENTUCKY, WITH A DESCRIPTION OF A NEW BLIND BEETLE.

BY H. GARMAN.

IT is common in writings on the origin of the cave fauna of the United States to assume that the recent formation of the caves of Kentucky is evidence of a similar recent origin of the blind animals which inhabit them. The geological evidence appears conclusive that the caves of the Green River region, and those to the northward in Indiana, were occupied during the Champlain period with water, and that their present inhabitants (at least the air-breathing species) must consequently have taken possession after the caves were elevated and the water no longer completely filled them. There can be no disputing the grounds for this belief; the geological evidence is all that could be desired for proof of a recent origin of the caves themselves. But I must beg leave to dissent from the conclusions which have been drawn from this proof, as to the recent origin of the blind animals. Conditions requisite to the development of eyeless animals are present in most parts of the United States. It seems only required that a species have no use for eyes, irrespective of the presence of light, and the eyes become reduced. Animals which burrow in the soil everywhere show a tendency to loss of the organs of vision. The moles, the worm-snakes (Carphophis), and *Cambarus bartoni* are familiar examples. Parasitic species lose their eyes, not in all cases because of a life in darkness, but because as parasites no eyes are needed. Numerous burrowing insects with poorly-developed eyes are known to occur over wide extents of territory. Beetles which live almost as exclusively in the dark as Adelops and Anophthalmus are not at all rare. Quite a list of non-cavernicolous blind beetles is known. It is to species such as these, already fitted for life in caves, that we should look, it seems to me, as representing the ancestors of cave species; certainly not to ordinary species with well-developed eyes. The originals of the cave species of Kentucky were probably already adjusted to a life in the earth before the caves were formed, and it seems probable from some facts mentioned below that they were not very different in character from the animals now living in the caves. I cannot believe that there has been anything more than a gradual assembling in the caves of animals adapted to a life in such channels. In this view of the matter the transformation of eyed into eyeless species appears to have been much less sudden and recent than has been supposed.

To take a definite example: There appears to be no imperative reason for assuming that the blind crustacean, *Cœcidotea* (*Asellus*) *stygia*, originated in Mammoth Cave. It was first discovered in caves it is true, but occurs widely distributed in the upper Mississippi Valley, is found throughout Kentucky, and is known to occur as far east as Pennsylvania. It is throughout its range a creature of underground streams, and is nowhere more common than on the prairies of Illinois (the last place in the country in which one would expect to find a cave), where it may be collected literally by the hundreds at the mouths of tile drains and in springs. In Kentucky, also, it is not more abundant in the cave region than elsewhere, being very frequently common under rocks in springs and in streams flowing from them, even during its breeding season. It is only natural that such a crustacean should have found itself at home in Mammoth Cave when this cavern was ready for its reception.

The blind fishes, again, are not by any means confined to the caves, but are widely distributed in underground waters throughout the country. *Amblyopsis spelœus* occurs in Indiana, Kentucky, and probably also in Missouri and farther south. *Typhlichthys subterraneus* occurs in Missouri, Kentucky, Tennessee, and Alabama. *Chologaster agassizii* occurs in Kentucky and Tennessee. *C. papilliferus* occurs in a spring in southern Illinois. I have had the pleasure of taking this species, and can say that the spring is evidently the outlet of an underground stream, and sends away a narrow but vigorous rill at all times of the year. *C. cornutus* I have taken, with the help of my friend, Professor B. P. Colton, in North and South Carolina, and can speak positively as to the situation in which it occurs. Like its relatives, it is a fish of underground streams, and makes its appearance at times at their mouths. Still another species appears, according to Dr. Packard, to have been observed in California. Here are widely scattered fishes with the family characters of Amblyopsis, and so probably closely resembling the eyed ancestors of the latter. They illustrate my point that there were in existence species possessing at least some of the characters of the Mammoth Cave forms when the caves became habitable; for it will hardly be supposed that all of these fishes originated in the caves of Kentucky and have become scattered since the glacial period. They illustrate, also, the point that hundreds of generations of a species may exist under the same conditions of environment as Amblyopsis and Typhlichthys, and yet not lose their eyes. Why then should these latter have had their eyes all but obliterated in the course of a few generations?

The distribution in this country of blind beetles of the genus Anophthalmus might at first thought appear to favor the idea that Mammoth Cave is a centre from which our species have been disseminated towards the East. Of our eight described species four (possibly five with *A. audax*) live in the Mammoth Cave region. Two others occur in Wyandotte Cave, only a short distance away. The single species not thus far recorded from these caves is *A. pusio* of Virginia and eastern Kentucky. It is to be remembered, however, that the large caves of Kentucky and Indiana have been much more thoroughly explored for cave animals than those of other parts of the country, and that their size and accessibility to Man have had much to do with the frequency with which they have been visited by collectors. They are simply portions of the haunts of the subterranean species which are opened up to us. One of the blind species (*Anophthalmus tenuis*) of Wyandotte Cave has now been found in Luray Cave, Virginia, a fact which gives us reason for believing that the Mammoth Cave species are more widely distributed than our present knowledge indicates. The large number of species (64) occurring in Europe points to that continent as the habitat from which all species of the genus have spread. If we accept this view of the origin of the genus then, whether the American species were introduced into this country before or after the Champlain period, it follows that our species have been but little modified by residence in Mammoth Cave, for if they had been we should find them departing more widely than they do from their European allies. They are in fact very closely related to European species. If we transfer the question of the sudden appearance of Anophthalmus to Europe, and claim still that the species are of post-glacial origin, that the eyes were lost suddenly after the Champlain period, we are met with the difficulty that here there is a gradation in both the habits and structure of the species which shows that the change may be and probably always has been gradual; for there are in existence species which live under rocks and have rudiments of external eyes.

Another aspect of this question of a sudden transformation of the species has recently been brought to my attention by some observations I have been making on the habits of these beetles, and particularly on a new species of Anophthalmus, of which a description is appended. Isolation in caves has been urged as an important factor in the development of those peculiarities by which cave animals are marked. It is assumed that the cave species are completely shut off from all relations with their out-of-door allies at an early stage in their phylogenic history. Nothing, it seems to me, can be farther from the truth. They are not even now isolated by anything except their inability to look out for themselves in the presence of their eyed enemies. *Cœcidotea stygia* is often found associated with *Asellus communis*, the eyed species from which it is supposed to have been derived. The cave cricket, *Hadenœcus subterraneus*, while occurring in the depths of caves, has always in my experience been found most abundant at the openings, where the twilight prevailing probably does not prevent the use of its well-developed eyes. It is frequently associated in such situations with its near relatives of the genus Ceuthophilus. Nor are the blind beetles confined to parts of caves in which total darkness prevails. Probably *Anophthalmus tellkampfii* is as completely adapted to a life in darkness as any of our species, and I have not yet found this species in the light; but I have found it abundant in a cave where

the rumbling of vehicles (not more than twenty feet away) passing on a road overhead could be distinctly heard. In all probability the beetles of this cave penetrate much nearer the surface than this. Some of the other species are common under rocks and wood in the shade of overhanging cliffs at the mouths of caves, where they are associated with the Carabidæ commonly found in such places. The isolation, such as it is, is largely voluntary on the part of the insects, and I can see nothing in the surroundings or habits which would indicate that they have ever been more completely isolated than they are now. I believe, on the contrary, that they are more completely isolated now, from specialization, than ever before.

In short, a reconnaissance of the zoölogy of Kentucky, which the writer has had an opportunity to make during the past two years, satisfies him that the evolution of the structures which characterize our cave species is to be considered apart from the question as to the age of Mammoth Cave, and that the origin of our aquatic cave fauna is in some respects a separate question from that touching the origin of the insect fauna.

Of these matters I hope to have something further to present in the future. Of the insects I may say now that there appears to have been after the Champlain period a migration towards Mammoth Cave of cave insects from the south and east, where the continent had not been so greatly affected by changes of level as was the Mississippi valley. Some observations in my possession tend to show that cave species are now abundant in the vicinity of the mountains of Eastern Kentucky. In fact much of the eastern end of the State appears to be adapted to an extensive subterranean fauna. It was a source of wonder to me during the first few months of my residence at Lexington how the rainfall disappeared so rapidly. A precipitation, which in central Illinois would have left its traces in muddy roads and swollen streams for weeks, disappears here in the course of forty-eight hours, having been swallowed up by a network of fissures in the underlying limestone and hurried down to the Kentucky River. These fissures are co-extensive with the Trenton limestone of this locality, and constitute the natural drainage system of the blue grass region of Kentucky. The wonder is not where the rainfall goes, but that any at all should remain at the surface. It early occurred to me that one might find cave animals in these fissures could he but get access to them. This can be done in some cases in quarries, and I can say as the result of preliminary exploration that some cave insects do occur here, and that at least one blind beetle is as abundant as it well could be. On a single visit to one of these opened channels I have, with the aid of a pupil, taken over one hundred specimens of the new species here described. It is without trace of external organs of vision, but like the earthworms possesses the power of recognizing light, a power which is evidently of some importance to it. It occurs in channels seemingly wherever there is food and moisture, and may be collected in the dim light near the openings. For some time I have kept forty individuals of this little beetle in my cellar where it appears to be perfectly at home, although during the day the light is never wholly excluded from its quarters. It wanders about freely, but may be sent scampering to cover by a flash of strong light. The food evidently consists of dead animal matter, such as insects and small mammals which are carried into fissures by freshets. This supply must be very great, though perhaps somewhat irregular; but this latter is a feature of the available food supply of many ordinary insects. Dead grasshoppers carried into the fissures are eagerly devoured. Food is evidently discovered by the sense of smell. In three minutes after placing a freshly killed grasshopper on the ground in one of the channels, several beetles were found at work on it. In confinement the beetles collect on such food after the manner of small ants, and eventually leave only the empty crust.

Anophthalmus horni, n. s. Somewhat depressed; smooth and shining; head, thorax, elytra, and abdomen everywhere provided with scant, erect, microscopic pubescence. Head oval; cheeks rounded; dorsal linear impressions rather deep; surface between the impressions very finely transversely rugulose; mentum tooth prominent, bicuspid. Antennæ densely pubescent excepting the thickened basal segment, which is smooth and shining, with a few hairs near its distal extremity. Thorax trapezoidal, larger than the head; sides strongly arcuate in front; sinuate behind; the hind angles acute but not produced; basal impressions deep, separated by a ridge at which the well-marked median linear impression terminates; truncate behind, but with a shallow emargination at each side separated by a wider median one; margin of contracted posterior part a trifle convex before the posterior angles. Elytra oblong oval, widest a little in front of the middle, truncate in front with the rounded humeri rather prominent; humeral margin obsoletely serrulate under a high magnifying power; striæ very evident next the suture, becoming obscure next the outer margin, obsoletely punctured, the third and fourth broken near the middle by a dorsal puncture, the sutural stria recurved at the posterior extremity of the elytron, joining the third; four rather strong punctures within each humeral margin, the second of which gives rise to one of the long setæ. Color pale fulvous, fading on posterior part of elytra to yellowish white, or cream color; curved impressions of head, edge of prothorax behind and at sides, rims about coxæ, etc., darker; length of body 3.67–4 millimeters; antennæ, 2–2.28 millimeters; length of head, 0.64 millimeter; width of head, 0.60 millimeter; length of thorax, 0.72 millimeter; width of thorax, 0.80 millimeter; width of thorax at base, 0.66 millimeter.

The species is closely related to *A. pusio*, Horn, from the Carter caves of eastern Kentucky, agreeing in size, in the absence of evident serrulation at the humeral margins of the elytra, and in the deep basal impressions of the prothorax. It differs in the size and shape of the prothorax, *A. pusio* having a very small prothorax, "not as long as the head and scarcely larger," whereas in this beetle the prothorax is distinctly larger than the head. The prothorax in *A. pusio* is as wide as long, and contracts in width somewhat gradually from the front, while in the new species this division of the body is broadly rounded at the sides, contracting rather abruptly behind. *A. pusio* is said to have pubescence only at the bases of the elytra. In this species the pubescence is rather scant, but is present on all the surfaces. The new species was discovered within the corporate limits of Lexington in the spring of 1890. It is named in honor of Dr. G. H. Horn of Philadelphia, who has contributed much towards an accurate knowledge of our species of Anophthalmus.

State College of Kentucky, Lexington, Oct. 8.

THE BOTANICAL LIBRARY OF A STATION BOTANIST.

BY A. S. HITCHCOCK.

PROBABLY the most essential part of the special equipment of a botanist to an experiment station is his working library. At least a part of the work of a station should be original investigation. In order that the results of his investigation should be an addition to the sum total of the world's knowledge, it is obviously desirable that the investigator should know all that has been published on the subject up to the time he presents his own results to the public. In the scientific world results are said to be known when they are put on record; that is, when they are published. If all the results of botanical investigation were published in one periodical, it would be an easy matter to hunt up the literature on a given subject. If all the results were to be found in botanical periodicals in the English, French, or German language, our work would be less easy, but still not difficult. But, lo! where must we look for our information? In botanical periodicals in all languages. I doubt if there be a station botanist in this country who can readily read all the botanical literature published in Europe. This statement will probably hold good if we exclude the Hungarian, Polish, and Russian; and most of us are confined to French, German, Latin, and possibly Italian. But this is not the worst; we must look through the proceedings of a multitude of scientific societies, prominent ones whose proceedings are readily accessible in the larger libraries, others more or less local and little known. But even this is not the worst; we find botanical literature in periodicals or proceedings devoted to general science, or even to miscellaneous matters. Sometimes it is tucked

away in a seed catalogue, a weekly agricultural paper, or even a college monthly. Fortunately there is a growing tendency to have articles reprinted and distributed more or less freely among contemporaries. In addition to these various channels of publication, we have the thousands of books, pamphlets, and sheets devoted more or less to botanical subjects.

It is obviously impossible for a station botanist to have ready access to even a tenth-part of the accumulated literature. It is only at the larger public institutions that an attempt toward completeness is made.

But in botany, as in other sciences, the period has long since been reached when classification of its literature was necessary. Thus with the proper aids it is possible for every botanist to become fairly familiar with the literature on any subject.

Probably there are as many opinions as there are station botanists as to the selection to be made of these aids, and it is the object of this paper to give one opinion out of the many.

First, as to the periodicals; assuming, as is generally the case, that the funds for library purposes are quite limited. Most of us take from our own country at least the *Botanical Gazette* and the *Bulletin of the Torrey Botanical Club*. The latter is especially useful for its "Index to Recent Literature Relating to American Botany."

Of foreign periodicals I would mention the *Botanisches Centralblatt*, for its "Referate," under which heading are given classified reviews of important articles, and for its "Neue Litteratur," which is an index, and a very complete one, to the current literature in all languages; the *Revue Générale de Botanique*, for its excellent reviews of the work done in various departments of botany during a given period; and the *Societatum Litterae*, giving monthly a classified list of articles published in the proceedings of scientific societies.

All will agree that by far the most important work is *Just's Botanischer Jahresbericht*. This gives an abstract, long or short, according to importance, of all the botanical articles published during the year. It is well indexed and classified.

Most of the station botanists are working more or less in special lines. The above-mentioned works will enable him to get at least the titles, and often an abstract of the contents, of nearly all the articles that have been published on his special subject. The most difficult period to cover is the last few months. *Just's Jahresbericht* is about two years behind, and the *Centralblatt* usually at least a few months.

Having at hand the titles and authors on a given subject, it is often desirable, or even necessary, to obtain the articles. Books, pamphlets, and reprints can usually be picked up through dealers in second-hand books. Separate numbers of the current periodicals and proceedings can usually be obtained. There remain such articles as are to be found only in the back numbers of serials. These are often very important and difficult to obtain. It is out of the question to think of purchasing these expensive works, for station libraries have too limited an income for this purpose. A good way is to be on the lookout for separate volumes containing the articles desired. But this requires some knowledge of the serials.

Three important works for this purpose and for botanical bibliography in general are Pritzel's "Thesaurus Literaturae Botanicae," Bolton's "Catalogue of Scientific Periodicals," and Scudder's "Catalogue of Scientific Serials."

After one obtains all the articles possible in this way, there will still be many that are unattainable. For these one must consult a large library. Short articles can then be copied, and notes can be taken of long ones. Photography will doubtless, in the future, play an important part in copying rare articles and plates. This can be done at a comparatively small expenditure of time and money, and has the immense advantage of being certainly correct.

I have said nothing about the selection of general works of reference and other books, as this depends so much on individual opinion and the line of work followed; but the above-mentioned aids to the botanist seem to me to be a necessary part of the equipment of every experiment station.

Agricultural College Experiment Station, Manhattan, Kansas.

FORENSIC MICROSCOPY.

BY L. A. HARDING, B.SC., PH.D.

FORENSIC Microscopy, like Forensic Medicine, has a close connection to law; it also deals with cases which are closely interwoven with the administration of justice, and with questions that involve the civil rights and social duties of individuals, the detection of poisons as well as the treatments of the recovery of poison from the poisoned. More and more in the history of the criminal courts is the demand occasioned for the application of the microscope, and microscopical toxicology. Although of late a certain line of medico-legal research has been obliged to combat with the works of the undertaker, who, when preserving the bodies of the dead, employs preservative compounds, largely composed of arsenical and mercurial compounds, while there is no question as to their preservative properties, yet the question arises, Is it good policy, is it for the good of the community at large, to employ embalming fluids composed of such poisonous substances? Criminals may easily hide their heinous crimes by having their victims embalmed, and who is there to tell which of the poisons was administered by the hand of the coward who did not dare to do his work before the world and openly, who for pecuniary or other reasons sought this road to remove a good man, nay, perhaps the man least to be spared, and who is there to identify the poison introduced by legitimate (?) means from that introduced with murderous designs? Yet, despite this opposing energy, despite the seemingly unsurmountable difficulties which surround forensic microscopy and toxicology on every side, we are still making progress and demonstrate that "forensic microscopy" is destined to be a branch of science which cannot be ignored, try as the opposers may.

If we measure the future by the work and benefits the microscope has done in the past, it will be seen that a very bright prospect is awaiting us indeed. No instrument yet devised by the ingenuity of man can compare with the microscope in its universal application to research in the broad domain of science, and I will endeavor in a brief way to call attention to a few of its special relations to law.

The direct application of the microscope to law dates back to about 1835, and ever since that time it has made a record for itself in convicting the guilty and protecting the innocent. The expedient taught to us by Albertus in 1226, that the victim's wounds would open afresh in the presence of the slayer, or the custom honored from time immemorial of watching the effect upon the suspected criminal as he touched the dead body of his supposed victim, we no longer are obliged to resort to. In the early age of forensic microscopy, its application was simply confined to a few questions of criminal law; but the more it attained perfectness in lenses, the excellent means of determining minute measurements, the adaptation of the spectroscope, and numerous valuable mechanical appliances, it has claimed so much attention in civil and criminal law that its usefulness cannot be denied. Although the microscope has played a very important part for a number of years in noted criminal and civil cases, its proper relation to law seems to be little understood. It is true that many underrate its value, and throw aside all testimony attained through its use as worthless, while others again largely overrate its powers. It is a well-known fact, though an unfortunate existing condition of affairs, that persons are permitted to give expert testimony in branches where they have but little more knowledge than the court before whom they testify. It is largely from this cause that so much discredit has been thrown upon the whole field of expert testimony, especially in this country. This condition of fact does not alone relate to forensic microscopy, but it has invaded all branches of expert testimony.

When, however, persons expert in the use of the microscope are called upon to give testimony, there ought not to be any disagreement as to the result of the examination they may make; as, for instance, if they examine a stain, and blood corpuscles are found by one, it should be verified by the other; and, if measurements of these corpuscles are made, their measures should correspond without a doubt. There should be no difference on such matters of fact, though this is not meant to imply that they should

not honestly differ as to how the blood came there. The microscope will tell with true and unerring certainty whether the adhering substance on a weapon is human or animal hair, or whether what is thought to be hair is not cotton, silk, or wool fibre. It is a well-known fact that portions of brain-substance adhering to weapons which have caused the fracture of the skull and laceration of the brain can only be recognized by the microscope. While, when the substance is fresh it cannot easily be mistaken, it is quite different when it becomes dry; it will then assume a gray or brown color, and become quite horny. In this state no physical appearance can tell what it is, the naked eye is at a loss to recognize its source. Quite different with the microscope; it will tell you. Moisten the substance, and you will see its color become whiter and its consistence quite soapy. Now if you soften the mass in a solution of common salt, I will show you nerve-cells or nerve-fibres; though so small, being only $\frac{1}{5000}$ of an inch or less in diameter, you shall see them plain and distinct. Likewise, hair adhering to clubs or weapons of any kind can be recognized as to its source, whether it has been torn out by force or not. If by force, we plainly see the tubular sheath of the hair, with the hair issuing from it; the color is distinguishable, the size, and whether they are cut at both ends or pointed at one, whether the bulb or sheath in which it grows is still attached to them, etc. It may not be amiss to state that hair from lower animals differs in a great many particulars from that of man; the hairs of animals, generally speaking, are coarser, thicker, shorter, and less transparent. The ones which bear a close resemblance to that of man are the spaniels and sky terriers whose hair is long and silky, though the linar markings on the cortial portions are not so numerous and fine. It is a deplorable fact that very little of value has been written upon the subject of hair in its medico-legal relation. While it cannot be denied that all the works on forensic medicine mention this subject, yet they are based upon very little original research; they are mainly copied one from another. We shall say more on this subject at a later date; we are willing to admit that it may not at all times be perfectly discernible as to the source of the hair, yet, when taken in conjunction with other evidence, doubt may be removed and positive evidence established.

It is understood, of course, that the examination of supposed weapons should be conducted with the greatest care, and notes taken, full notes in fact of all the detail and every process in the operation; especially spots and marks which can have any possible bearing upon the case under question should be carefully noted.

In the broad domain of chemistry and toxicology the microscope is a very important factor for the identification and verification of many ordinary tests, which are made to determine the composition of solids and liquids. Not many years ago, death from poison was surrounded by dread and fear scarcely comprehensible at the present day. Tradition informs us that persons suspected of having committed murder by poisoning were broiled alive in England, and in France burned at the stake, and in the various other countries tortured in the most inhuman manner. It is now, however, generally conceded that, with modern methods introduced for the detection of poison, the fear of discovery has been rendered greater than the dread of punishment. The greatest advance in legal chemistry was through the achievements of Bunsen and others; quantities so minute as to be out of reach of all other known methods of analysis, we are enabled to identify with unerring certainty. Many poisons, such as strychnine, arsenic, morphine, etc., will crystallize with certain reagents into characteristic forms, which are peculiar to themselves.

Of late considerable attention has been paid to the microscopical examination of hand-writings. While perhaps the microscope cannot be considered an aid in forming an opinion as to the real author of a given specimen, yet its value for the detection of alteration and changes made in the original cannot be underrated. It is impossible to make an erasure of any written or printed lines and hide them from detection by the microscope; the most skilful forger cannot restore the slightest derangement of the fibres on the finished surface of the paper.

Equipped with the modern improvements and possessing the requisite skill, the progressive microscopist may be said to be a true friend of the curious, in the full meaning of this expression. It is true that sometimes our most exhaustive means of industry and research are only rewarded by negative results; yet it cannot be denied that in the majority of cases we reap the reward of diligence and industry by seeing our work change the whole theory of a plea in civil and criminal action, becoming a terror to the guilty and joy to the innocent.

THE tenth congress of the American Ornithologists' Union will convene in Washington, D. C., on Tuesday, November 15, 1892, at eleven o'clock, A. M. The meetings will be held at the U. S. National Museum. The reading of papers will form a prominent feature of the meetings. Associate as well as active members are earnestly requested to contribute, and to notify the the secretary before November 12 as to the titles of their communications and the length of time required for their presentation, so that a programme for each day may be prepared.

— Among the articles of the November number of *The Forum* is one on "The Library of the United States" by Mr. Ainsworth R. Spofford, Librarian of Congress, who explains the rank that this great library will take among the great libraries of the world. In the series of articles on Municipal Government there appear two contributions in the November number: 1, by the Rt. Hon. Joseph Chamberlain, who compares the Government of Birmingham, England, with the Government of Boston, and tries to ascertain why Boston's government costs five times as much as Birmingham's, they being cities of about the same size; and 2, by Mr. Charles Francis Adams, who points out lessons from the municipal experience of Quincy, Massachusetts. Professor Edward S. Holden, Director of Lick Observatory, tells what we really know about Mars. In the series of articles giving the results of his investigations into our public-school system, contributed by Dr. J. M. Rice, the November number contains his study of the schools in Buffalo and Cincinnati.

— Mr. O. P. Hay has furnished for recent "Proceedings of the National Museum" three interesting biological papers. The first is entitled "On the Ejection of Blood from the Eyes of Horned Toads," and establishes beyond question the fact that under certain conditions about the time of moulting *Phrynosoma coronatum* ejects from the eye a small quantity of blood. Mr. Hay records personal observations on the toads, and also quotes the experiences of others. Professor L. M. Underwood furnishes the following account: "In 1885 a student of mine received a specimen of horned toad from California. In examining the animal I took occasion to turn him on his back, using a lead pencil for the purpose. The animal resented this treatment, and showed considerable anger, opening his mouth and puffing up his body. On being irritated still more, he grew more and more enraged, until finally blood spurted from just above his eye to a distance at least a foot from the animal, as several spots struck my arm considerably above the wrist. After spurting the blood the animal became limp and collapsed, and remained in a stupor for some time; and when handled behaved as if dead. After a time, possibly not over five or six minutes, certainly not over ten, the animal revived and commenced to run about the table. Wishing to know if he would repeat the operation, I commenced to irritate him again in the same manner. After becoming enraged again, the animal soon went through the same process, ejecting blood from the same eye as before. He then fell into a similar stupor and remained about the same length of time, after which he revived. No amount of irritation could produce a third discharge, although the animal showed some anger." Mr. Hay also records "Some Observations on the Turtles of the Genus Malaclemys," and presents a number of interesting facts concerning "The Breeding Habits, Eggs, and Young of Certain Snakes." No. 905 of the Museum "Proceedings" consists of a valuable paper by Mr. L. O. Howard on "The Insects of the Sub-Family Encyrtinæ with Branched Antennæ." Three new genera (*Pentacnemus, Tetracladia, Calocerinus*) and species are described, five species being figured.

SCIENCE:

PUBLISHED BY N. D. C. HODGES, 874 BROADWAY, NEW YORK.

SUBSCRIPTIONS.—United States and Canada .... $3.50 a year.
Great Britain and Europe .... 4.50 a year.

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Attention is called to the "Wants" column. It is invaluable to those who use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

BOGDANOV ON THE PRIMITIVE RUSSIANS.

BY JOHN BEDDOE, LL.D., F.R.S.

THE Anthropological Congress lately held at Moscow, however much its attractions and its attendance may have been diminished by the cholera scare, has at least produced one very notable and interesting paper—that by the veteran Professor Anatole Bogdanov, entitled "Quelle est la race la plus ancienne de la Russie centrale?" In it Bogdanov recalls the fact that twenty-five years have elapsed since he published his first researches into the subject on which he now delivers a fairly matured opinion. During a great part of the interval he has been laboring in this field and collecting material, not from the centre only, but from all parts of Russia, though at times he seems to have abandoned the effort for a while in a kind of despair.

His earlier researches led him to form the opinion that the kurgans (tumuli) of central Russia, believed to date from the ninth up to even the fifteenth century, contained the remains of two races, one dolichocephalic, tall and strongly made, with light-brown hair, the other smaller, with short, broad head and dark-brown hair. The former he found preponderated in the earlier kurgans, and in the south-western part of the central provinces, the latter at later dates and more to the north-east. In spite of the mode of location, but in accordance with the apparent dates, those who considered these facts mostly agreed that the dolichocephals were of Finnish kindred, Merians probably, and that the shorter heads belonged to the Slavs who invaded and incorporated them.

Later discoveries and the products of a wider field do not, in Bogdanov's opinion, confirm this view. These long skulls, which, though the occiput projects considerably, have usually well-developed frontal regions, and are by no means of low type, are found to prevail in the older interments throughout the west and south as well as the centre of Russia, while short heads abound in the north and east, in the ancient kurgans of the Uralian region and in those of the Bashkir territory. Bogdanov inclines to the opinion of Poesche, that the Slavs "descended in reality from a dolichocephalic source." And, seeing that the modern Slavs are on the whole moderately brachycephalic, he thinks that the prevailing form has somewhat changed through contact and crossing with races having broader heads (meaning probably the Mongoloid races which lie and have lain to the east of them), but also owing to the operation of other (external) causes. "With the progress of civilization," he says, "begins another series of influences, which has played a great part in the history of peoples, and may play a still greater one in the future, because the conditions of civilization bring about necessarily in the course of time an increase of brachycephalism. . . . Dolichocephalism declines more and more in Europe, and the heads become larger and finer."

Thus does Bogdanov range himself on the side of the short heads in the curious controversy which is arising in Europe as to the relative merits of the two leading forms of cranium, and to which Obedenare, Laponge, and Von Ammon have contributed both facts and opinions. I recollect asking Professor Rokitansky, five and thirty years ago, whether the Czechs were not brachycephalic. Rokitansky was himself a Bohemian, and he was evidently nettled by a question which he thought touched upon a weak point in his fellow-countrymen. "Ah! well!" he said, "they are a very clever people for all that." On the other hand, Messrs. Jacobs and Spielmann, in their recent paper on the physical characters of British Jews, almost apologized for the long-headedness (in a physical sense) of the Sephardim, as a mark of inferiority! Since Topinard claimed the Aryan language as the original property of the short-headed Kelto-Slavo-Galcha family, their congeners have taken heart, and threaten to push us long-heads from our stools of conceit.

Whence came these aboriginal dolichocephals of Russia? "Not from Asia or the Caucasus," says Bogdanov. "It is more likely that they came from the Danube, where we find at present dolichocephaly predominant [in Bulgaria]. They probably followed the Dnieper into White Russia, thence to Novgorod and into Sweden. This was the northward stream. About the same time there was probably an eastward current through Minsk to Yaroslav and Moscow, and a western one by Galicia, the Vistula, and the Danube."

ON "TYPE-SPECIMENS" AND "TYPE-FIGURES" IN ENTOMOLOGY.

BY W. F. KIRBY, LONDON, ENG.

A "TYPE SPECIMEN" is the specimen of an insect from which the original describer drew up the first description of a species; and it is often of great importance to settle disputed points of nomenclature, where any doubt exists respecting the actual identification of a species; for if we are certain that we have the original specimen before us, no further dispute is possible. A "type-figure" is the figure quoted by the original describer as illustrating his species, or is a figure supposed to represent the species published by a later author.

This appears plain enough; but in practice it is not always satisfactory. The specimens described by the older authors, such as Linné and Fabricius, are not always in existence, and in other cases it is not always certain that the specimens in various old collections supposed to represent the types of these authors are actually the real specimens which they described. Again, Linné frequently quoted several figures of different species as illustrating one of his species; and, in several other cases, he seems to have described quite different species in his successive works. Under these circumstances it does not follow that a specimen, even if ticketed by Linné himself, is necessarily the species which he originally described. Some of the later authors, too, such as Müller and Hontheim, have figured insects as species of Linné, and applied wrong Linnean names to their figures in the most reckless manner.

In the case of Fabricius, we already meet with far more careful and conscientious work; and when Fabricius describes an insect from a known locality, there is often very little doubt about what he really intended. But his names, too, were frequently misapplied by his contemporaries; and it is only lately that several insects which he described from India, but which his contemporaries mistook to refer to European species more or less resembling them, have been correctly identified. Gross errors, too, have been committed by certain recent authors who have found specimens of insects supposed to have been named by Fabricius in old collections, and have jumped to the conclusion that they were his original types, though neither the locality nor the description may have applied to them at all. This does not apply to collections indubitably referred to by Fabricius, such as the Banksian and Hunterian, which may usually be regarded as authoritative.

Again, some authors have cared more for the condition of their specimens than for scientific accuracy, and may in some cases have actually got rid of their own types and replaced them with better specimens, possibly of a different species more or less re-

sembling the real one; this, apart from errors or transposition of labels, to which accidents all collections are more or less liable, in proportion to their age.

While, therefore, fully admitting the great value of a type, or type-figure, it is necessary to ascertain that it is really the specimen or represents the specimen originally described. If it contradicts the original description in any important respect, and especially if it is an insect known to be from a different locality to that assigned to it by the original describer, it is more than probable that it is not the original type at all, and is worse than misleading. Errors of locality are always possible; but much will depend on the author. Donovan, for instance, was extremely careless about localities, but, as he figured all his species, this matters less; on the other hand, Fabricius was far more careful than later authorities have given him credit for; and an error of this kind in his work was quite exceptional.

THE CONVEX PROFILE OF BAD-LAND DIVIDES.

BY W. M. DAVIS, HARVARD COLLEGE, CAMBRIDGE, MASS.

In Mr. Gilbert's analysis of land sculpture, constituting chapter V. of his "Geology of the Henry Mountains," he explains why the surface of an eroded region possesses slopes that are concave upwards and steepest near the divides, and shows that it is for the reasons there stated that mountains — that is, mature and well-sculptured mountains, such as are of ordinary occurrence — are steepest at their crests (p. 116). The *arêtes* of the Alps illustrate this perfectly. Gilbert calls this generalization the "law of divides."

But in discussing the forms assumed by eroded bad-lands, or arid regions of weak structure with insignificant variety of texture, he finds an exception to the law of divides. The two lateral concave slopes of a bad-land ridge do not unite upwards at an angle, forming a sharp divide, but are joined in a curve that is convex instead of concave upwards. "Thus in the sculpture of the bad lands there is revealed an exception to the law of divides,— an exception which cannot be referred to accidents of structure, and which is as persistent in its recurrence as are the features which conform to the law,— an exception which in some unexplained way is part of the law. Our analysis of the agencies and conditions of erosion, on the one hand, has led to the conclusion that (where structure does not prevent) the declivities of a continuous drainage-slope increase as the quantities of water flowing over them decrease; and that they are great in proportion as they are near divides. Our observation, on the other hand, shows that the declivities increase as the quantities of water diminish, up to a certain point where the quantity is very small, and then decrease; and that declivities are great in proportion as they are near divides, unless they are *very* near divides. Evidently some factor has been overlooked in the analysis,— a factor which in the main is less important than the flow of water, but which asserts its existence at those points where the flow of water is exceedingly small, and is there supreme" (pp. 122, 123).

It has for some time seemed to me that the overlooked factor is the creeping of the surface soil; and, as I have not seen mention of this process as bearing on the form of the crest-lines of divides, a brief note on the subject is here offered.

The superficial parts of rock-masses are slowly reduced to rock-waste or soil by the various processes included under the term, *weathering*. Unconsolidated materials are in the same way reduced to finer texture near their surface. The loose and often fine material thus provided at the surface is carried away by various processes, of which the chief are moving water, moving air, and occasionally moving ice; but there is an additional process of importance, involving dilatation and contraction of the soil, and in consequence of which not only the loose particles on the surface are transported, but a considerable thickness of loose material is caused to creep slowly down-hill.

Dilatation is caused by increase of temperature, by increase of moisture, and by freezing. Vegetable growth may probably be added to this list. The movements are minute and slow. They are directed outwards, about normally to the surface. Contraction follows dilatation, when the soil cools or dries, or when its frost melts. The movement of the parts is then not inward at a normal to the surface, but vertically downwards, or even downwards along the slope. As the two motions do not counterbalance each other, a slow down-hill resultant remains. This is greatest near the surface, where the dilatations and contractions are greatest; but it does not cease even at a depth of several feet, perhaps of many feet. Hence the down-hill dragging of old-weathered rock, often well shown in fresh railroad cuttings in non-glaciated regions. I presume all this is familiar to most readers; although from the frequent inquiry concerning the means by which valleys are widened it is evident that the creeping process is not so generally borne in mind as that by which running water washes loose material down-hill.

The form assumed by the surface of the land depends largely on the ratio between the processes of washing and creeping. Wherever the concentration of drainage makes transportation by streams effective, the loose material is so generally carried away (except on flood-plains) that the action of creeping is relatively insignificant. But on divides, where drainage is not concentrated but dispersed, the ratio of creeping to washing is large, even though the value of creeping is still small. This is especially the case in regions of loose texture and of moderate rainfall; that is, in typical bad-lands, where the supply of loose surface-material ready to creep is large, and where the loose material is slowly taken away by washing. On the divides of such regions, the surface form is controlled by the creeping process. The sharp-edged divides, that should certainly appear if washing alone were in action, are nicely rounded off by the dilatations and contractions of the soil along the ridge-line. The result thus determined by the slow outward and downward movements of the particles might be imitated in a short time by a succession of light earthquake shocks.

Mr. Gilbert has himself given several beautiful illustrations of the close dependence of sharp or rounded divides on rainfall; structure remaining constant. If the rainfall should increase in bad-land regions, would not all their divides become sharper; and if the rainfall were continuous, so as to carry away every loose particle as soon as it is loosened, would not the divides assume the sharp ridge-line expected from Mr. Gilbert's analysis but not found in the actual arid bad-land climate? In the eastern and well-watered part of our country, I have often seen clay-banks much more sharply cut than the equally barren surface of the western bad lands; but even on clay-banks, the minute divides between the innumerable little valleys are not knife-edge sharp; they are rounded when closely looked at. Perhaps they are sharper in wet weather and duller in dry spells.

If rainfall remain constant and structure vary, then the harder the structure, the less the supply of soil for creeping and the sharper the divides; the weaker the structure, the more plentiful the supply of soil for creeping and the duller the divides. Numerous examples of this variation might be given.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Some Remarks on the Botanic Trinomial.

AN article in *Science* for September 16, signed C. H. Tyler Townsend, contains certain statements which cannot be passed, it seems to me, without some few words of discussion. It is quite evident that this article loses sight entirely of the main purpose of a biological name, and seems to imply that the name of a thing has to do with justice, right, etc. For example, I find therein the following expressions: "In no case can the name of the original erector and describer of a genus be separated therefrom without gross injustice." "There is no necessity whatever for shedding glory upon the one who has made the transfer. . . . He has no right whatever to the species." These words, "injustice," "right," belong to the field of Ethics, not that of Taxonomy.

I shall try to consider the botanic trinomial, not from the ethical point of view as Mr. Townsend seems to have done, but from the taxonomic strictly.

We find it convenient to give a name to a plant simply because the use of the name serves to call up an aggregate of characteristics when we wish, without the necessity of detailing those characteristics. The whole matter is one of convenience simply, and a name means nothing more than this.

It has been pretty universally agreed that it is more convenient to have a binomial name than a monomial one, for by this means we are enabled easily to group our plants, the first name serving to call to mind the aggregate of characteristics of the group (genus), possessed often by many sub-groups (species), and the second those characteristics possessed to a greater or less extent by the individuals that go to make up the sub-group.

So far this seems to be reasonable enough, and, following the same lines, should we choose to add a third name to our binomial, making it a trinomial, we should naturally do so for the purpose of segregating these sub-groups into still smaller ones (varieties). On this line the addition of terms might rationally be continued to the extent that the facts of observation would warrant.

But we find in the *de facto* botanic trinomial a mixture of two taxonomic principles, instead of the rational following out of the single line indicated by adding to the monomial the second term. Usually the third term is added as a compromise with existing fact, simply to avoid the possibility of having two homonomic binomials, and consists of the name of the person who first published the binomial. It is evident that this addition of such a third term serves a purpose only in comparatively rare cases; in the vast majority, were it not for the fear that some future comer would see fit to use the same binomial to designate another plant, it would be, as a name, useless. But at present the addition of the author's name is essentially a part of the naming of the plant.

It is this third name, and comparatively useless one, that is the cause of much of the trouble of the botanic taxonomists. Many seem to feel that this serving as a compromising tailpiece, the necessity for which it is confessedly the aim of the botanic world to do away with altogether, is an honor. And for this reason there is strife in a large class of cases as to the third name to be added to the binomial. For consider the following specific case. Hooker and Arnott notice a plant, which, in their judgment, is a member of the large group of plants that has been called Malva. They therefore give it the binomial name *Malva malachroides*, and first publish the characteristics which that name is to call up. Afterward Gray considers that the plant cannot belong to the group called Malva, and so gives the same plant the name *Sidalcea malachroides*. More recently Greene finds that the plant can be neither a Malva nor a Sidalcea, and calls it *Hesperalcea malachroides*.

Now suppose we have an individual of this group and wish to give it the most convenient name. For the name of the main group undeniably it matters not which of the three names we choose; if we have had the opportunity of studying the plant carefully our choice will be determined by the observed facts and our own judgment. Personally, in the present case, I chose to call the plant Hesperalcea. For the second name there is no choice, the three authors having given it the same. (Had there been a diversity of names here, the name first given the plant would have been chosen, not because this is "just," or "right," but because by this artificial rule we obtain a permanent factor in the name, without fossilizing individual opinion at all regarding the affinities of the plant.)

We now come to the third name, only added, remember, from the fear that some one has called or will call some different plant Hesperalcea malachroides. Here custom is divided, and many would write *H. malachroides, H. and A.*, and others *H. malachroides, Greene*. It is for us now to determine which of these names is the most convenient.[1] The person to whom we wish to communicate the idea, H. malachroides, upon seeing the trinomial H. malachroides, H. and A., naturally turns to the works of H. and A. to find the summing up of the characters of the plant. But here he is met with an insurmountable difficulty. He can find no trace of it. Let him look for *malachroides*, perchance Mr. Townsend would say. But it is easily possible that H. and A. have described five species by the name of *malachroides*. On the other hand, suppose we write H. malachroides, Greene, the person wishing to know of this plant would turn to the works of Greene and there would find the reference to Malva malachroides, H. and A., which would enable him to find the original description of the plant and thus obtain the idea which we wished to convey.

It seems plain enough then that the third name of this trinomial from the standpoint of convenience should be Greene and not H. and A..

Mr. Townsend disposes of this difficulty in the following words:—

"I would write Metsgeria pubescens schrank, . . . and make no more ado or trouble about it. . . . This signifies always that the authority named described the species originally and originally proposed that name. The founder and date of the genus can be ascertained by referring to any monograph."

It is obvious on a little thought that this paragraph assumes a good deal more than the facts warrant. In the first place there certainly will be no monograph of the species named *pubescens*; and it is very possible that a monograph of the generic name chosen may not exist.

But it is perhaps allowable to look at these two trinomials from a slightly different point of view. Which tells the most truth? H. malachroides, H. and A., implies that H. and A. would now choose, as we have done, the group Hesperalcea for this plant. This we have no right to imply; as a matter of fact they did choose Malva, and this is all we know or should state.

Of course, in all the preceding I have assumed that the purpose of a name is to convey from one person to another the idea of a thing, and on this hypothesis it seems to me that the conclusions arrived at are sound; but I would not wish to be understood as desiring that a name should do no more than this. If it can convey the history of the thing, well and good, as long as by trying to do this it does not entirely defeat its own purpose, as I think I have shown Hesperalcea malachroides, H. and A., would do.

C. MICHENER.

San Francisco, Oct. 7.

[1] I have not considered the writing of H. malachroides (H. and A.) Greene, as the parenthetical term is no more an essential part of the name than the date of publication or twenty other particulars which might occur in a monograph on the plant.

Notes on the Saturniidæ, or Emperor and Atlas Moths.

ALTHOUGH the family *Saturniidæ* comprises the largest and some of the handsomest of all the *Lepidoptera*, it is still very imperfectly known. The larvæ are mostly gregarious, and feed on trees. Many of them form cocoons, which are attached to the branches of the trees upon which they live, while others (at least in South Africa) are said to pupate in the ground. I am not certain whether it has yet been ascertained whether this latter habit has been proved to be peculiar to certain species or genera, or whether the same species may form its pupa in different ways, according to circumstances.

There is doubtless a much greater variety of these insects in tropical countries than we are at present aware of. Many of the most remarkable species are only received singly, and often remain unique in our collections for years. Collectors rarely have an opportunity of rearing them from the larvæ, even if they should meet with a brood, and many species probably feed on lofty trees, quite out of reach, while the perfect insects are nocturnal in their habits. Many of the larger, and especially the domesticated species of *Saturniidæ* from which silk is obtained in India, China, and Japan, vary very much, and this is another obstacle to their successful study. Many of these domesticated breeds, and the various wild or semi-domesticated forms allied to them have been simply named, and not described; or perhaps only the food-plants and localities have been indicated. These useless names find their way into our collections and from thence into our lists and papers, and form a wholly unnecessary element

of confusion, which should be eliminated as soon as possible, either by the actual description of the species, or by the rejection of these manuscript names. The mischievous practice of attaching names to insects without describing them has long been abandoned by lepidopterists in every branch of the study except sericiculture.

W. F. KIRBY.

London, England, Sept. 25.

Destroying Mosquitoes by Kerosene.

THE reason for the existence of mosquitoes has often been asked. Some means for their destruction has, perhaps, been even more earnestly sought after. The idea that their numbers can be kept down by propagating dragon-flies does not seem to be any longer entertained; and any experiment bearing on some means for their destruction is of interest. In a late number of *Insect Life*, Mr. L. O. Howard publishes a note upon the use of kerosene against them, the substance of which is as follows: On the surface of a pool of water, containing about 60 square feet, he poured four ounces of kerosene. This formed a very thin oily film on the surface of the water. On the 5th of July the pool was teeming with animal life, but for the next ten days that the pool was under observation no living insects were observed. At the end of this time, a count of the insects on a small portion of the surface, from which was estimated the total number, showed 7,400,—370 of which were mosquitoes. The observation is of interest as showing the remedy to be an effective one, and, further, that a single application of oil will remain operative for ten days or longer, although two rain storms occurred during the interval. The matter is worthy of further observation and experiment.

JOSEPH F. JAMES.

Washington, D.C., Oct. 10.

Phonetics in Science.

FOLLOWING almost in the "wake" of the geological word-makers, who have apparently a dictionary of their own construction, comes another scientific writer who has decided to use the phonetic system of orthography. My attention was called to an article in a chemical journal published in this country, and almost at a glance I should have decided, had I not known the system, that the author had just finished writing a translation from the Spanish, and had his alphabet somewhat confused; for here before me was *sulfate;* but reading further, I should have said, perhaps, that he had just finished a German translation.

All this would have occurred to me if I had been ignorant of the existence of the phonetic system. Now, why did not this author change *phenol-phtalein*, which appears in the article referred to? Perhaps this word does not occur in the phonetic dictionary.

Is it not high time for American scientists to stop "coining" words? To be sure, these words differ from the geological ones in that they come well recommended by some philologists, and then the author in this case has not been guilty of owning an "orthographic mint." Why not continue to use the good old spelling, when it answers every requirement? The only disadvantage (?) in so doing, to my mind, may be in the fact that the words are longer than those in the phonetic system, and, as the advocates of this system claim, are more difficult to spell; so they are to some people, but unless they are foreigners, one is not in the habit of meeting such scientists in every-day life. Scarcely has our American language secured a strong foot-hold than it must be changed for the benefit of a few who would receive the honors as the originators and champions of a new system of orthography. I know of one advocate (not the author, it is needless to say, of the paper in the chemical journal above referred to) who "prides himself not only upon his ability to use the phonetic system, but also upon his beautiful English." Yet this very same man habitually uses, for example, such phrases as "Ain't he funny?" Still this hardly belongs to my criticism of phonetics in science. Why not leave the phonetic system to the philologists; why incorporate it in our scientific work?

When the advocates of this system have succeeded in establishing a strong foot hold for their system, and permanency (for it) stares the old system in the face—and let us hope that time is far distant—then we can almost picture our laboring scientists, with the new system (?) dictionary before them, ever fearful of beginning one word with an F after the new, and the next with a Ph after the system they have so successfully used for generations.

E.

Grand-Gulf Formation.

DR. WM. H. DALL'S contribution to Miocene literature under this head calls for some notice, were it only to thank that eminent palæontologist for correcting my mistake with regard to the Gnathodon of Pascagoula and Mobile. With his unrivalled opportunities of comparison and long experience in these studies, his determination is naturally satisfactory and final. I knew that in mollusks the young and the adult forms often differ considerably; but I knew not the life history of this one.

It is complimentary to me also that he has accepted my outline of the evolution of the Florida Peninsula,[1] although he probably arrived at his conclusions from different and independent sources. And I wish to correct the impression he seems to have of my notions of the genesis of the Grand Gulf. I do not say that the Pascagoula is a deep-sea formation, but speak of it as a "marine aspect" of the more intensely fresh-water Grand Gulf on the Mississippi; and I do not suppose that in an estuary marine influences prevail over the fluviatile, in order to foster the life of any of the creatures that have left their remains in these calcareous clays and sands; so that it may be said to be "partially of marine genesis." The same views here expressed by Dr. Dall were indicated by myself in another paper published by the Geological Survey of Alabama on the "Nita Crevasse" in 1889, in which I speak of the progress of later formations on and in the Mississippi Sound and its older extension as presenting a "marine-aspect" of the "Port-Hudson group" of Dr. Hilgard, and sufficiently different to be called the Biloxi Formation—a nomenclature I understood to have been approved by him among others. The method of genesis sketched in that paper for the Port Hudson was considered applicable to the older Post-Eocene formations of the same embayment.

I do not perceive, therefore, that Dr. Dall's "correction of my definition of these clays" was "required;" nor have I any to make of his, for similar views have been elaborated for the forthcoming Alabama Geological Report, which will be in effect a new edition of Bulletin 37 of the United States Geological Survey.

The only criticism here to which Dr. Dall might seem amenable is a tacit endorsement of his own brochure of January last upon these same Miocene formations, in which it may be said he has permitted conjecture upon general principles somewhat to outrun and forestall positive discovery. Hasty generalization is the bane of science. The Pascagoula Clays may be equivalent to his Chesapeake, but the testimony as yet can scarcely be said to be satisfactory. Whilst he has shown the younger Miocene of northern Florida, originally named by me the Waldo Formation, phases of which are seen at White Springs, in Hamilton County, and in the overlying clays at Aspalâga on the Apalachicola River, to be Chesapeake; this surely cannot be identical with the upper layers at Alum Bluff, much less with the lower.[2] As he himself has shown, the latter is an older Miocene, identical with that occurring on Chipola at Bailey's Bridge, and called by myself Chipola at a time when, from high water, I had not seen the *Ortholax beds* at Alum Bluff, and when I had not seen the perfect instance of contact and overlap presented at that place. At that time, I had previously discovered a Miocene in the vicinity of Defuniak Springs, on Shoal River, and on Alaqua River (and named it from the last), tracing it across Choctawhatchie, near Knox Hill, and across Washington County a little south of Vernon, and across Chipola at Abe Springs, eight miles south of Ten-Mile Bayou, the principal site of the older Miocene. With the help of Mr. Jüssen (both of us then working with Mr. Geo. H. Eldridge on the geological

[1] See Dr. J. W. Spencer's First Report of the Geological Survey of Georgia, p. 60; and short papers of my own, read severally at the meetings of the Geological Society of America, August, 1891, and August, 1892.

[2] There is no fossiliferous formation at Hawthorne, nor any at Ocheesee, as Dr. Dall seems to suppose.

survey of Florida) the differences between these two formations was established, and for the younger the name of Aliqua revives. Whether this is identical with the Chesapeake and Carolinian or not is for another discussion. At the same time the same parties identified the Chattahoochee beds of Langdon, which underlie the Miocenes of Georgia and northern Florida, with the Chipola beds, and traced their continuity westward across the Choctawhatchie, until, meeting with the syncline of the great roll from Alabama, they sink out of sight under the great sand-beds which fill the depression now drained by Shool River.

The connection of these two Florida Miocenes with the eastward extension of the Grand Gulf into south Alabama is matter for field research, and cannot be decided in the closet upon general principles. Enough is certain, however, to render it clear that if it is proper to draw the line between an older and a younger Miocene in Florida, such a distinction continues westward into Alabama and Mississippi; and where can we draw it better than upon lithological grounds between the water-holding stratified sands and sandstones of the lower Grand Gulf and those overcapping clays which, pierced at Brewton and Pallard 70 feet, at Mobile 735, at Biloxi 770, at Pearl River 800, and at New Orleans 1,200 feet, yield similar flows of water with similar clays and fossils? Of the latter I have other collections, which shall be submitted to Dr. Dall, now that I know his attention has been turned to the matter.

Upon the use of the term *formation*, I finally have to say that it is at least provisional, for every discoverer to name every structure he finds having peculiarities from some locality where it is prominently developed, although in the course of palæontological research many of these provisional names may disappear; and I submit that the prevailing American practice is not an abuse. For these reasons I shall still insist upon the propriety of calling the Pascagoula Clays the Pascagoula Formation.

LAWRENCE C. JOHNSON.

Meridian, Miss., Oct. 2.

Jealousy in Infants.

OF my two children one is a boy of four years, the other a girl of ten months. The boy has just returned home after an absence of some months. His sister displays great affection for him. She is also much attached to her nurse, more so at times apparently than to any other member of the household.

Now if, while the girl is sitting on a mat alone or on the lap of either of her parents, the nurse should take the boy upon her knee and fondle him, the girl will immediately cry out in a distressful way, in a tone not precisely indicative of anger or vexation, but more nearly similar to the tone of grief or disappointed desire. In the case described the infant will not be appeased unless the nurse puts down the boy and takes her up. It will not avail for the nurse to take her up on one knee, leaving the boy on the other.

If, however, while the nurse has the infant in her arms, either of the parents takes up the boy and caresses him, the girl displays only a strong interest, but no annoyance whatever.

It is evident then that the outburst of feeling in the former case was a display of jealousy. And, as the child is not precocious, it is allowable to look upon this case as an instance of ordinary mental development in children.

It is wonderful enough that infants of a few weeks or months should make unmistakable manifestations of the simpler emotions of fear, affection, and anger. But that an emotion so complex as jealousy should appear so early as at the age of ten months is especially remarkable, and indicates a degree of development at this age which, in the absence of observation, might justly be deemed incredible.

I have not by me the works of Taine, Preyer, or Perrez, and so am not able to say what observations, if any, they made in respect to this particular matter. Darwin observed jealousy in an infant of fifteen and a half months, but adds, "it would probably be exhibited by infants at an earlier age if they were tried in a fitting manner."

A. STEVENSON.

Arthur, Ontario, Canada.

Is There a Sense of Direction?

THE recent articles in *Science* by Dr. Hall and Dr. Work on this subject tempt me to say that in early life I was a believer in this sense, my belief being derived from Cooper's Leather Stocking Tales and similar sources. The winter of 1855-56 was spent in what was then called "the bad-axe country" of western Wisconsin, in company with an old French-Canadian trapper, who seemed to possess this gift in a (to me) marvellous degree; and, as he boasted of it and never to my knowledge made a mistake, my belief in this sense was confirmed.

The next winter, with a very limited knowledge of the Ojibwa tongue, picked up on the Bad Axe, I went with a government survey into northern Minnesota in the capacity of interpreter. Here the subject was discussed in camp, and the sceptics proposed a test. Five Indians were blind-folded, turned around several times, and led half a mile from camp in different directions. Not one could point to the camp until the bandage was removed from his eyes, nor could they point to the north. As soon as they could see they easily found the camp, although it was in the flat, low-rolling country north-east of Crow Wing, where there are no prominent land marks to be seen from the heavy-timbered lands. On several other occasions it was found that the Ojibwa was guided by the lie of the land, as indicated by water-courses, the twist of trees as seen on stubs denuded of bark, the sun, and the many minor indications of the cardinal points that are known to expert woodsmen, both white and red. Therefore I agree with Dr. Hall that man does not possess an instinct which teaches him to find his way to a given point regardless of darkness or of previous knowledge of locality.

I cannot agree that any animal possesses this sense. If so, it would be the wild animals, whose necessities would keep the sense in training, and not those whose needs have been supplied by man. Dr. Hall cites the cat, which has been taken in a box for fifty miles and yet reached home. This may be so; but such instances, if true, are recorded as wonderful, as they truly are; while the thousands of other cats which were taken less than five miles from home and never returned are never recorded. Dr. Work mentions the many carrier pigeons which never return, and it is generally conceded that these birds depend on sight alone, their trainers taking them short distances at first, and then increasing them until they know the way to the loft.

Let us take the case of the greatest of all migrating animals, the wild goose. All of us who have seen anything of these birds have seen them lost in a fog. Dr. Work thinks their flying at different altitudes may be determined by "the character of the upper currents," and if these currents determine the density of fogs, he is right; for on a clear day, when the geese can see many miles ahead and get a bird's-eye view of landmarks fifty miles distant, they fly very high, but let rain or mist prevail, and they drop within reach of gun-powder, because they must come near the earth to get their bearings and preserve the direction of their flight, by vision alone.

I have, among my flock of wild fowl, a pair of brant, *B. bernicla* (the only goose that Atlantic coast gunners call "brant," although in the West every goose is a "brant," except the Canada goose). One of these birds strayed from a flock going north in the spring of 1890, during one of the darkest of nights, when the rain came as hard as rain can come, and was captured while flying around a street-lamp in the village, thoroughly bewildered. The other was taken the same night two miles south of the village by a boy who found it on the ground. Such instances are common in every rural locality, not only with the "black brant," but with its larger relative the Canada goose as well; and if there are better navigators in the animal world who should have the "sense of direction," if there is such a sense, I do not know what animals they are.

Dr. Work covers the case in his last paragraph, when he says: "Whatever instincts animals may have in this direction, man has the same, with the additional faculty of reason." That is, he covers the question of a "sense of direction" in animals, and allows man as much; but I cannot subscribe to his implied assumption of reason by man alone. That, however, is another question.

FRED MATHER.

Cold Spring Harbor, N.Y.

Monstrous Poppy.

THE monstrous poppy described by Mr. Clark in *Science* for Oct. 7 is one of pistillody rather than "gynandry," and it is by no means so new a thing as he supposed. Masters (*Veg. Teratol*, p. 304) describes and figures similar monstrosities, and refers to Goeppert, who, as long ago as 1850, "found numerous instances of the kind in a field near Breslau." This pistillody of the poppy is mentioned also by Frank (*Krankheiten der Pflanzen*, p. 350), who reproduces Master's figure. CHARLES E. BESSEY.

University of Nebraska, Lincoln.

Yeasts as Expounded in the "North American Review."

WHY does Mr. Lockwood revive the old idea that yeasts "beget moulds?" In an interesting but inaccurate article entitled "The Hygiene of the Atmosphere" in the *North American Review* for this month there is the following paragraph: "Omnipresent in the atmosphere are the invisible spores of the fungi, know as the *Torulacei*. They beget many of the mould and mildews seen on decaying vegetation. Some of these act also as ferments, decomposing vegetable and animal matter. Of this group, for good and evil, the air almost everywhere contains the spores of *Torula cerevisiæ* or yeast fungus, literally the mother of vinegar, alcohol, and leavened bread."

The classical researches of Brefeld and Hansen have long ago exploded the notion that the yeast plant is only an immature form of a species of mould. The terms *Torulacei* and *Torula* are also out of date, *Saccharomycetes* and the generic name *Saccharomyces* being mostly used at present. It is true there is some diversity of opinion as to the systematic position of the yeasts. Some think they constitute a distinct class; the majority of botanists believe, however, that they are degenerated forms of the *Ascomycetes*. There is absolutely no reason for the statement that the mother of vinegar is another form of the yeast fungus. They are by no means different stages of the same plant, and are only related in that they are both fungi. Hansen has proved that *Saccharomyces erevisiæ* and *Saccharomyces pastorianus* are beer ferments, and that *Saccharomyces ellipsoidens* is the wine ferment. *Mycoderma aceti* occasions acetic fermentation. Chemically these processes are even more distinct. The former converts certain carbohydrates into alcohol and other products with the evolution of carbon dioxide; while by means of the presence of *Mycoderma aceti* alcohol is oxidized into acetic acid or vinegar. By means of the solid culture media, gelatine and agar agar, introduced for the cultivation of bacteria, white, black, and pink yeasts have been carefully studied, principally by Hansen. Besides budding or gemmation there is another mode of reproduction in the yeasts. The protoplasm of the cell forms spores, and the cell-wall becomes an ascus. They are therefore called ascospores, and the yeasts are considered degraded ascomycetes. JOHN GIFFORD.

Swarthmore College, Pa., Oct. 8.

BOOK-REVIEWS.

Man and the Glacial Period. By G. FREDERICK WRIGHT. New York, D. Appleton & Co. 1892. 8°. 385 p. Ill.

As a glacialist, the author of this volume stands among the first in this country, and his long study of that remarkable period in the geologic history of our planet invests all he says about it with uncommon authority. In his work, proceeding in a true scientific manner from the known to the unknown, he first describes the main existing glaciers in various parts of the world, and devotes a chapter to the physics of glacial motion. Summing up the signs of past glaciation, he examines separately the ancient glaciers of the Western and of the Eastern Hemispheres, describes at considerable length the drainage systems both in America and Europe, and directs especial inquiry into the cause of the glacial period and its probable date.

All this is well done, and supplies the most compact and satisfactory exposition of our knowledge of the subject which has yet appeared, — the facts carefully stated and the opinions maturely formed. To a very important chapter, and the one which for many readers will be the most interesting in the book, such unreserved praise cannot be extended. This is the chapter on the "Relics of Man in the Glacial Period." The author believes there are such relics both in Europe and America, and that they have been discovered and proved. No one will deny that there may be such; it is likely enough; but that any such relics have been found under conditions which remove all doubts as to their authenticity and age is open to considerable question.

Confining our attention to examples in the United States, let us see what is offered. His first instance is the rough implements found by Dr. Abbott in the Trenton gravels. But these gravels are unquestionably post-glacial, and no one can say how much *post*. The late eminent glacialist, Dr. Carvill Lewis, considered them rather modern, and also maintained that what Dr. Abbott believed to be undisturbed layers, were those of an ancient talus. These statements Dr. Lewis made at an open meeting of the Academy of Natural Sciences, Philadelphia, not long before his regretted death, concerning specimens from Dr. Abbott which I then laid before the Academy. It is the opinion of most glacialists that the Trenton-gravel finds require further study before we can assign their probable age. I have myself found these chipped stones in the Trenton talus, but never in clearly undisturbed strata.

Dr. Wright's next examples are the finds of rough implements, in the glacial gravels in Ohio, by Dr. Metz, Dr. Cresson, and Mr. Mills. The two first-named are eminent archæologists, but neither is a geologist, and it may as well be accepted once for all that no opinion as to the age of a gravel can be received from any but an expert geologist, one who has specially studied this most difficult subject. Not one of these finds, therefore, is conclusive.

The next example offered is the discovery of flint chips and implements in the alleged glacial gravels by Miss Babbitt, near Little Falls, Minnesota. This locality has been re-examined this year by members of the Bureau of Ethnology, with the result of proving that the implement-bearing layer is unquestionably modern, and not glacial, nor post-glacial.

Next, the alleged implements from the Columbia gravels at Claymont, Del., are adduced. These gravels are far older than the last glacial action, and it would indeed be wonderful were they deposits of human industries. I can say that the discovery of such in them is wholly rejected by McGee and Holmes, who have closely compared all the evidence; and I add that the supposed implements from them which I have examined show no sure signs of human workmanship; while the argillite pieces certainly come from a talus.

The remains under Table Mountain, California, which are next brought forward, have been unanimously denied by archæologists any great antiquity. They belong to a modern industry, and in all probability were left in their shafts by the aboriginal gold-diggers a few centuries before the conquest. The manner of their deposition alone proves this, and the case is given up by Professor Haynes, in his excellent Appendix to Dr. Wright's book.

Dr. Wright's last example is the feeblest of all — the Nampa image, a "beautifully-formed clay image of a female," said to have been brought up from a depth of 320 feet (!) in the boring of an artesian well, at Nampa, Idaho. It is sad to destroy illusions; but when this same image with its story was laid before a well-known government geologist, and he at once recognized it as a clay toy manufactured by the neighboring Pocatello Indians, the person displaying it replied with engaging frankness, "Well, now, don't give me away!"

These are Dr. Wright's evidences of glacial man in America. It will be seen that his structure is rather slight. Very much more solid evidence than any yet brought forward will be necessary to establish this most important fact. D. G. BRINTON.

AMONG THE PUBLISHERS.

"THOUGHTS of Busy Girls" is the title given to a volume of short essays from the pens of working girls, which Miss Grace M. Dodge, the well-known philanthropist, has edited and prefaced. These essays are quite remarkable, considering the disadvantages

under which the writers worked. While they may occasionally trip in their grammar, they show intelligence and thought and have the merit of having been written with all seriousness of purpose. They should act as a stimulant to other working girls. The Cassell Publishing Company will issue the book.

—Messrs. D. Appleton & Co's list of autumn announcements includes, among other titles, "An Attic Philosopher in Paris," by Emile Souvestre, illustrated by Jean Claude, and uniform with "Colette;" "The Story of Columbus," by Elizabeth Eggleston Seelye, edited by Dr. Edward Eggleston, with one hundred illustrations by Allegra Eggleston; "Three Centuries of English Love Songs," edited by Ralph Caine, with frontispiece after Angelica Kauffman; "Abraham Lincoln, the Story of a Great Life," by William H. Herndon and Jesse W. Weik, with an introduction by Horace White, and many illustrations; "Admiral Farragut," by Capt. A. T. Mahan, and "Zachary Taylor," by Major-General O. O. Howard, U. S. A., the first two volumes in the Great Commander Series, edited by General James Grant Wilson; "Man and the Glacial Period," by Professor G. Frederick Wright; "Along the Florida Reef," by Charles Frederick Holder, illustrated; "Warriors of the Crescent," by W. H. Davenport-Adams, illustrated, and uniform with "Pictures from Roman Life and Story," by Professor A. J. Church; "North America, Vol. III., the United States," by Élisée Reclus; "Modern Mechanics," a supplementary volume to Appleton's Cyclopædia of Applied Mechanics, illustrated, edited by Park Benjamin, LL.B.; "Appleton's Atlas of Modern Geography," with maps and illustrations of all countries; "Idle Days in Patagonia," by C. H. Hudson, C. M. Z. S., author of "The Naturalist in La Plata;" "Moral Instruction of Children," by Felix Adler, a translation of "Rousseau's Emile," by W. H. Payne, Ph.D., LL.D., and "English Education in the Elementary and Secondary Schools," by Isaac Sharpless, in the International Educational Series; and new editions of "An Englishman in Paris," in one volume; "Lecky's History of England in the Eighteenth Century," in twelve volumes, of which five are devoted to Ireland; Herbert Spencer's "Principles of Ethics," Vol. I.; Huxley's Essays upon some "Controverted Questions;" Tyndall's "Fragments of Science;" and the authorized edition of the "New Drill Regulations of the United States Army."

—The Quarter Centennial Programme of the Kansas Academy of Science, held at Atchison, Oct. 12, 1892, contained the following papers: The Descent of Facial Expression, A. H. Thompson, Topeka; Notes on the Distribution of Kansas Rushes and Sedges, M. A. Carlton, Manhattan; Notes on Ampelopsis quinquefolia Michx., Variation, E. B. Knerr, Atchison; The Relations of the Composite Flora of Kansas, A. S. Hitchcock, Manhattan; A List of Flowering Plants and Ferns collected in Franklin County, Kansas, during the months of April, May, September, and October, 1890-92, W. E. Castle, Ottawa; Some Ornamental Kansas Stones, S. W. Williston, Lawrence; The Analysis of Kansas Building Stones, E. H. S. Bailey and E. C. Case, Lawrence; Some Notes on Condensed Vegetation in Western Kansas, Minnie Reed, Manhattan; The Organization and Work of Local Scientific Clubs, T. H. Dinsmore, Emporia; On the Horse Flies of New Mexico and Arizona, C. H. Tyler Townsend, Las Cruces, N. M.; Note on Peculiar Acalyptrate Muscid found near Turkey Tanks, Arizona, C. H. Tyler Townsend; The Characteristics of the Glacial Area of North-east Kansas, Robert Hay, Junction City; Kansas Niobrara Cretaceous, S. W. Williston, Lawrence; The Variation in Chemical Composition of Plants Collected at Different Seasons of the Year, Illustrated by the Common Dandelion, L. E. Sayre, Lawrence; Joseph Savage — A Memorial, Robert Hay; Astronomical Phenomena in 1892, T. H. Dinsmore; Notes on a Pink Barite Found in Atchison Limestone, with Analysis, E. B. Knerr; Notes on Colorado "Mountain Leather," with Analysis, E. B. Knerr; Notes on Comparative Insect Anatomy (a Laboratory Guide), V. L. Kellogg, Lawrence; Insect Notes, V. L. Kellogg; An Interesting Food Habit of the Plesiosaurus, S. W. Williston; The Archæan Area of Missouri, E. Haworth, Lawrence; On Para-chlor-meta-

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

Oct. 15.—Obituary notice of Mr. A. O. Aldis, by Mr. Jos. K. McCammon; Obituary notice of Mr. William Ferrel, by Mr. Cleveland Abbe; Obituary notice of Mr. J. E. Hilgard, by Mr. O. H. Tittmann; Obituary notice of Mr. C. H. Nichols, by Mr. J. M. Toner; Obituary notice of Mr. C. C. Parry, by Mr. F. H. Knowlton; Obituary notice of Mr. George Bancroft, by Mr. J. B. Marvin.

Biological Society, Washington.

Oct. 22.—The principal topic of the evening, Botanical Nomenclature: (*a*) The Present Status of Botanical Nomenclature, by F. C. Coville; (*b*) Report on the Botanical Congress at Genoa, by Geo. Vasey; (*c*) Some Controversial Points in Botanical Nomenclature, by George B. Sudworth. Other communications: Discovery of Fossil Plants in the Potomac Formation, at the New Reservoir, Washington, D.C., and at Mount Vernon, and Discovery of a Second Specimen of Saul's Oak (Quercus Prinus + alba), by Lester F. Ward; The Fauna and Flora of Roan Mountain, North Carolina, by C. Hart Merriam.

Engineer's Club, Philadelphia.

Oct. 1.—Strickland L. Kneass, The History and Development of the Injector; Carl G. Barth, Distribution of Pressure in Bearings.

Publications Received at Editor's Office.

FOREL, F. A. Le Léman, Tome I. Lausanne: F. Rouge. 8°. Paper. 536 p.
HARVARD GRADUATES' MAGAZINE. Vol. I., No. I., Oct., 1892. Boston, The Harvard Graduates' Magazine Association. 8°. Paper. 176 p. 50c.
LODGE, OLIVER J. Lightning Conductors and Lightning Guards. London, Whittaker & Co. 12°. 556 p.
MONTMAHON AND BEAUREGARD. A Course on Biology. Translated by Wm. H. Greene. Phila., J. B. Lippincott Co. 12°. 368 p. 75c.
U. S. DEPARTMENT OF AGRICULTURE. Report of the Chief of the Weather Bureau for 1891. Washington, Government. 8°. Paper. 91 p.

nitro-benzyl-chloride and its Derivatives, E. C. Franklin, Lawrence; Harmonic Forms, B. B. Smyth, Topeka; The Effect of Some Grass Diseases on the Tissue of the Host, W. C. Stevens, Lawrence; Some Useful Apparatus for the Botanical Laboratory, W. C. Stevens; On Echinacea angustifolia, S. R. Boyce, Lawrence; Additions to the Flora of Kansas, B. B. Smyth, Topeka; Catalogue of the Kansas State Herbarium, B. B. Smyth; Additions to the Minerals of Kansas, G. H. Failyer and E. H. S. Bailey; The Yield of Extractive of Various Vegetable Substances, L. E. Sayre; A Breathing Well in Logan County, Kansas, J. T. Willard, Manhattan; The Improvement of Sorghum by Seed Selection, J. T. Willard; A Résumé of the Current Opinion on the Structure of the Nervous System, C. Judson Herrick, Ottawa; Maximum Binding Moments for Moving Loads in a Swing Beam, E. C. Murphy, Lawrence; Notes on some Kansas Weeds, A. S. Hitchcock; The Distribution of the Genus Vitis in Kansas, A. S. Hitchcock; Notes on the Occurrence of Granite in a Deep Boring in Eastern Kansas, Robert Hay.

—Macmillan & Co. are to publish very shortly a work by Dr. S. S. Laurie entitled "The Institutes of Education, comprising a Rational Introduction to Psychology." Dr. Laurie is author of "Occasional Addresses on Educational Subjects," "Lectures on Language and Linguistic Methods," etc., and writes from long experience on the meaning and importance of a science of education.

—Macmillan & Co.'s October announcements include, The Beauties of Nature, and the Wonders of the World We Live In, by Right Hon. Sir John Lubbock; Charing Cross to St. Paul's, by Justin McCarthy, illustrated by Joseph Pennell, new edition; The English Town in the Fifteenth Century, by Alice Stopford Green; Sketches of Life and Character in Hungary, by Margaret Fletcher; History of Federal Government, From the Foundation of the Achaian League, General Introduction, History of the Greek Federations, new edition; Life in Ancient Egypt, translated from the German of Professor Erman by Mrs. Tirard; A Defense of Philosophic Doubt, by the Right Hon. A. J. Balfour, being an Essay on the Foundations of Belief, new edition; The Theory of Wages, and its Application to the Eight-Hours Question, by Herbert M. Thompson; A text-Book of Pathology, Systematic and Practical, by Professor D. J. Hamilton, Vol. II.; A Text-Book of Physiology, Part V.—Appendix, The Chemical Basis of the Animal Body, by A. Sheridan Lea; Metal Coloring and Bronzing, by Arthur H. Hiorns; Hygiene, by Edward F. Willoughby, new and enlarged edition; Scientific Papers, by Oliver Heaviside, two volumes; Pioneers of Science, by Professor Oliver Lodge, with portraits and other illustrations; Hereditary Genius, An Inquiry into its Laws and Consequences, by Francis Galton, F.R.S., new edition; Chemical Theory for Beginners, by James Walker and Leonard Dobbin; Materials for the Study of Variation in Animals, Part I., Discontinuous Variation, by William Bateson; On Color Blindness, by Thomas H. Bickerton; The Great World's Farm, or, How Nature Grows Her Crops, by Selina Gaze, with a Preface by Professor Boulger; Text-Book of Embryology, Man and Mammals, by Dr. Oscar Hertwig, translated and edited from the Third German Edition by Dr. E. L. Mark; Text-Book of Embryology, Invertebrates, by Drs. Korschelt and Heider, translated and edited by Dr. E. L. Mark and Dr. W. McMichael Woodworth; The Photographer's Pocket-Book, by Dr. E. Vogel, translated by E. C. Conrad; Physiological Psychology, by Dr. Th. Ziehen, adapted by Dr. Otto Beyer and C. C. Vanliew; The City State of Greek and Roman Antiquity, by W. Warde Fowler; Atlas of Classical Antiquities, edited for English use by Professor W. C. S. Anderson; Principles of Elementary Algebra, by Nathan F. Dupuis; Introductory Modern Geometry of the Point, Ray, and Circle, by William B. Smith; The Elements of Graphical Statics, A Text-Book for Students of Engineering, by Leander M. Hoskins; A Review of the Systems of Ethics founded in Evolution, by C. M Williams; and Theory of Functions, by James Harkness.

TO THOSE INTERESTED IN SCIENCE.

[Science (weekly) established in 1883, N. D. C. HODGES, 874 Broadway, New York City.]

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